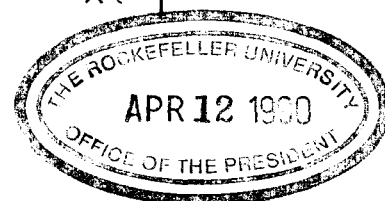


THE JOHNS HOPKINS UNIVERSITY
INSTITUTE OF THE HISTORY OF MEDICINE
1900 EAST MONUMENT STREET
BALTIMORE, MD. 21205



April 7, 1990

Dear Dr. Lederberg:

Thank you so much for your letter referring to my "History of Immunology." I am pleased that you enjoyed it, although I am conscious of having made many errors of omission and of commission. But someone had to make a start on this complicated story. I have now left my lab for the address above, where I will continue my historical hobby, concentrating now on some aspects of the sociology and philosophy of science where I feel that immunology may shed some light. Incidentally, I wonder whether you recall our having met at the Pasteur centennial symposium?

Now to the questions that you raised in your letter. I have read the material that you sent, and was especially impressed by how little Niels Jerne has changed over the years (as judged from a comparison of his 1955 letter to you with my own recent correspondence with him). I will write him, to recall the letter, and to discuss it further. You express surprise that I conclude that "clonal selection took the increasingly biologically-oriented world of immunology by storm." You are of course correct that the Paulings and Haurowitz's did not then and never would accept clonal selection; I was in communication with Felix until his death, and he certainly never converted, nor has to this day Alain Bussard, one of the last of the old-line immunochemists. And of course, the vocal response at the time came from the opponents of the theory, as would be expected. Those who looked upon it with favor supported it only indirectly in their research programs, a much more subtle form of commentary.

Thus, I think that you were hearing from the wrong crowd! The world in general did not become fully aware of the theory until 1959, when Burnet published his book and you and Dave Talmage published your Science papers. You cite the 1967 Cold ^{hard} Spring Harbor meeting as the watershed, but I submit that this ~~was~~ really occurred at the 1964 Prague Symposium which, for the first time was predominantly biological in content. It was here that Burnet rose to proclaim the triumph of his theory, and the majority of the audience concurred. I was there, and remember it well. So was Niels, and Gus Nossal, and I am sure that both will bear out my memory of the occasion. Surely such a victory (among the biologists) in only five years almost justifies my hyperbole.

I hope that you will write up the story of the dynamics of the theory's reception, as you threaten to do. When you do it, I hope that you will consider publishing it in the History of

Immunology series in Cellular Immunology, which series has been well received by the community, and assures a wide readership.

I am now working on a study of the dynamics of conceptual change in the history of immunology, dealing with the two major gestalt shifts that occurred: the first, a devolution from the original bacteriological-medical era to that of the chemical one, about the time of the First World War, and the second, from immunochemistry to immunobiology, that we have discussed above. I hope that you will permit me to send you a preprint for your comments.

Yours very truly,

A handwritten signature in cursive script, appearing to read 'A. Silverstein'.

Arthur M. Silverstein

copy: Niels Jerne